Are Export Support Programs Effective? Evidence from Tunisia[#]

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December 12, 2011

Abstract

The paper combines firm-level datasets from Tunisia's customs, National Statistical Institute, and the FAMEX matching grant program to evaluate the effect of export promotion on the export performance of beneficiary firms. Propensity-score matching difference-in-difference and weighted least squares estimates suggest that beneficiaries expand at the intensive and the extensive margin (markets and products). However, this expansion does not translate into higher exports for beneficiaries beyond the program enrolment year. We show that existing exporters coming to FAMEX to expand into new markets or to develop new export products seem to have benefited more and that very small and relatively large grants have no significant effect on export performance. Our evidence suggests that although treated firms significantly diversified, they failed to transform this diversification into reduced exposure to price risk on their portfolio of export markets. We also show weak evidence of negative spillovers from treated to control firms.

JEL classification codes: F13, F14, L15, L25, 017, 024, C23.

Keywords: Export promotion, firms, export margins, Tunisia, impact evaluation, propensity-score matching.

[#]We are grateful to Daniel Lederman and Mine Senses for useful comments and discussions, to Yasusuki Todo for sharing his matching methodology, to Helmut Fryges for his do-files for the GPS methodology and to participants at CEPII, the Conference on Trade and Firms in Aix en Provence, July 2011 and the DECTI-IGC October 2011 workshop on "Trade policy for growth" in Washington DC for comments. This study was supported by the governments of Norway, Sweden and the United Kingdom through the Multi-Donor Trust Fund for Trade and Development. The findings expressed in this paper are those of the authors and do not necessarily represent the views of the World Bank.

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1. Introduction

With the decline in the use of traditional, trade-restricting instruments of trade policy over the last two decades, governments in the developing world have increasingly turned to programmatic interventions aimed at enhancing the competitiveness of domestic firms. These interventions include for example export promotion, export-processing zones, border-modernization programs, and single windows for customs' administrative procedures. How effective are these interventions? Assessing their degree of success is important on at least two counts. First, they mobilize funding from national governments and donors with a potentially high opportunity cost. Second, assessing their impact (or absence thereof) helps to understand the binding constraints to developing country export growth. In particular, export promotion, the subject of this paper, specifically addresses informational problems. Assessing its success may say something about how severe is the informational constraint on exporters.

What do we know about the effectiveness of export promotion? The literature has developed along two strands. One strand —the oldest— relies on cross-country evidence in search of effects on aggregate export performance. For instance, Rose (2007) uses a gravity equation to show that diplomatic representations had a positive effect on bilateral trade flows. Lederman, Olarreaga, and Payton (2010) use aggregate export equations to show that, after a long history of failure in developing countries largely due to misguided flanking policies like import substitution and currency overvaluation, export promotion agencies have recently had more success in increasing aggregate exports, in particular agencies whose management involves the private sector.

Another strand—more recent—has has looked for effects of export promotion using quasi-experimental methods, comparing the export performance of treated firms with that of a control group. Since enrolment into export promotion programs is never random, most papers go to great lengths to control for selection effects through matching, fixed effects, and two-step (IV or Heckman) estimation methods.[1] The results are mixed, depending, in part, on what performance variable is considered, e.g., export status, diversification, or intensive margin growth. Bernard and Jensen (2004) show an insignificant effect of state-level export promotion expenditures on the probability of exporting for U.S. manufacturing plants. Alvarez and Crespi (2000) conduct a survey of 365 Chilean firms, of which 178 received export assistance in several forms, including access to a business information system and participation in international fairs. Using a two-step approach to control for selection, they show a significant impact only on the number of export destinations; neither product diversification nor overall export growth at the firm level are

[1] Cadot, Fernandes, Gourdon, and Mattoo (2011) indicate that the World Bank is considering running randomized control trials with export promotion schemes targeted at individual entrepreneurs (in particular women) in low-income countries, providing them with mobile phones, electronic payment systems for cross-border payments, and price information.

affected significantly. Görg, Henry and Strobl (2008) use a propensity score matching difference-in-difference (PSM-DID) estimator on Irish data combining plant-level export variables with other characteristics to examine the impact of subsidies. They find that large enough subsidies encourage the export activity of already-exporting firms. However, they find little evidence that non-exporters are encouraged to start exporting by subsidies of any size once unobserved plant effects are controlled for by first-differencing. Volpe and Carballo (2008) also use PSM-DID to explore the effectiveness of Peru's export-promotion program, PROMPEX, using Peruvian firms' customs data. They show that PROMPEX encouraged export growth at the extensive margin (destinations and products) but not at the intensive margin. Girma, Gong, Görg and Yu (2009) use IV techniques on a large panel of Chinese firms to explore the effect of *production* subsidies on exporting activity. They obtain robust evidence of a positive impact at the intensive margin, but little evidence of an encouragement-to-export effect on initially non-exporting firms. Finally, Volpe and Carballo (2010) explore the distributional effects of PROCHILE's export promotion program using Chilean firms' customs data and find stronger positive effects for small firms.

All in all, the evidence so far suggests two remarks. First, export promotion seems to be more successful at affecting the performance of established exporters than at encouraging non-exporting firms to start exporting. This is in accordance with the literature on heterogeneous firms and trade, which suggests that exporters differ from non-exporters in terms of productivity and a host of other characteristics (see, e.g. Bernard, Jensen, Redding and Schott 2007). After all, one could hardly expect export promotion to change ducks into swans. Second, with the exception of Girma, Gong, Görg and Yu (2009) who consider the special case of production subsidies, the evidence seems to be stronger for impacts at the extensive margin than at the intensive margin. This is somewhat natural, and perhaps even desirable, if the information hurdles to break into new markets (product- or destination-wise) are larger than to simply ramp up export volumes.[2]

Underlying the debate about the effectiveness of export promotion as a public policy is a key—but largely un-discussed—assumption, namely that there is a market failure. But the fact that potential exporters are not fully informed about foreign market opportunities is not sufficient, in itself, to create a market failure if information production is costly but appropriable. A market failure could arise in the presence of imperfect appropriability of the information. Indeed, Volpe and Carballo (2008), citing McDermott (1994), note that "customer lists are the most common target of corporate spies." The work of Rauch (1996, 1999) and Rauch and Trindade (2002) also stresses the importance of networks and externalities in the search for foreign trade partners. By contrast, in a recent World Bank survey of African exporters (Cadot, Iacovone, Pierola and Rauch, 2011), competitors are cited only marginally as sources of first contacts with foreign customers, and Bernard and Jensen (2004) find little evidence of cross-firm spillovers in their

[2] See Rangan and Lawrence (1999) and references therein on the hurdles facing the internationalization of firms.

study of the determinants of the decision to export. Thus, at this stage, the notion that export entrepreneurship creates externalities that need to be supported by public action, discussed in Hausmann and Rodrik (2003), is still largely an open question. Alternatively, the market failure could arise from a dysfunctional credit market, if adverse selection or moral hazard prevented trustworthy exporters from obtaining credit. In the first case (information as a public good), the absence of a treatment effect from a program aimed at reducing information problems in export markets could be the result of non-appropriability rather than ineffective treatment, and should therefore be interpreted cautiously. Only in the second case (adverse selection or moral hazard) could the absence of a treatment effect from the program be safely interpreted as treatment failure. In this paper, we will attempt to tackle the issue of externalities directly.

Against this background, we explore the impact of Tunisia's export promotion program, FAMEX, which consists of a matching grants provided to firms to implement an export business plan. We combine several sources of firm-level data—FAMEX program data, national statistical institute and investment promotion board data, and customs transaction data—into into a unique, rich dataset of Tunisian exporters. The advantage of using merged customs data with other data sources is that with customs data there is no recall (or other) bias in the outcome variables compared to the use of firm-level surveys which is a standard approach used to evaluate public programs ex-post. In the case of FAMEX, the World Bank collected firm-level survey data to analyze the impact of the program and the corresponding analysis is described in Gourdon, Marchat, Sharma, and Vishwanath (2011).

We use a menu of estimation methods, including PSM-DID and weighted least squares (WLS) regressions to estimate the FAMEX treatment effects, and we extend the analysis in several directions relative to previous studies. First, we test to what extent the effect of export promotion is sustained over time. We find that, compared to a control group, FAMEX beneficiaries successfully diversify in terms of destination markets; however, the rate of growth of their total exports diverges only temporarily from that of the control group. After one year, the growth rate of FAMEX beneficiaries' total exports slows down and converges back to that of the control group. We provide tentative evidence suggesting that although treated firms significantly diversified, they failed to transform this diversification into reduced exposure to price risk on their portfolio of export markets.

We dig deeper into the evaluation of the FAMEX program in two directions. First, we examine whether the impact of FAMEX differs depending on the firm's objective in requesting assistance. We find that existing exporters coming to FAMEX to expand into new markets or to develop new export products seem to have benefited more than firms coming to FAMEX to increase their exports, a result that is consistent with existing evidence in other settings. We explore the effect of various components of the program and find that prospection and promotion activities (missions abroad, acquisition of data, conception and production of marketing material) correlate more significantly with export outcomes than other types of services like assistance to new-product development or to organizational change. This finding suggests that informational barriers are the most amenable to effective government assistance. We also explore the matching-grant dimension of the program by estimating a "dose-response" function following Fryges (2008) and Fryges and Wagner (2008) and find that very small grants and relatively large grants have no significant effect on export performance.

Finally, we examine the importance of externalities by estimating the impact on the performance of control firms of the number of treated firms in the same industry and geographical area. This is an important—though typically underexplored—part of program impact evaluation, because in the presence of informational spillovers, the absence of a positive measured treatment effect could mean a positive *true* treatment effect combined with positive externalities on the control group, precisely the combination that would justify government intervention. As it turns out, our evidence is weak and if any suggests *negative* instead of positive spillovers, which may reflect poaching of good managers and workers by treated firms (which receive cash from the program in the form of matching grants).

The paper is organized as follows. Section 2 describes the export promotion program and Section 3 presents the data. Section 4 discusses estimation issues. Section 5 presents our main results. Section 6 discusses robustness and extensions, and Section 7 concludes.

2. Export Promotion in Tunisia

Since the mid-1990s the Tunisian government has attempted to reduce the traditional anti-export bias of Tunisia's trade policy (World Bank, 2008). Among several measures, including the elimination of tariffs on imported raw materials, equipment and capital goods in many sectors, the Tunisian government expanded its use of export promotion tools to help exporters get access to foreign markets. The World Bank supported Tunisia through a loan for the Export Development Project (EDP) which was implemented in two phases: the first phase lasted from 2000 to 2004 and the second phase - which is the object of our analysis - lasted from 2005 to 2009. The *Centre de promotion des exportations* (CEPEX - Export Promotion Centre) was the key agency under the Ministry of Trade responsible for implementing Tunisia's export promotion activities.

Our analysis focuses on a key program under the EDP project, the Second Export Market Access Fund which includes a matching grant fund - FAMEX - to help Tunisian firms overcome barriers to sell in foreign markets on a demand-driven basis. The FAMEX program also helped to build the institutional capacity of local professional organizations, such as export associations, chambers of commerce, and professional consulting organizations, to enable them to support more efficiently Tunisian exporters.[3] The rationale for putting the FAMEX program in place was the pervasive lack of information on export markets by Tunisian firms which had difficulty in identifying the right target market, the right product segment, and the right selling channel.

The FAMEX program provided non-reimbursable matching grants to co-finance 50 percent of the cost of firms' investments in market research and pre-competitive programs to increase their export market access and competitiveness. By taking the form of a matching grant program, FAMEX would expect a low probability of misallocation of funds and of support for low-value services and a high commitment of firms to their projects due to their high contribution to the costs.[4] The minimum annual firm turnover for FAMEX eligibility was TND 200,000 (144,000 USD) in manufacturing and TND 100,000 (71,000 USD) in service and craft sectors.[5] In theory FAMEX was supposed to accept applications only from firms that had been in operation for a minimum of two years, but there were exceptions.

A firm approaching FAMEX for assistance had to submit an export business plan focused on one of three main objectives: (i) to start exporting if the firm had little or no export experience, (ii) to diversify its destination markets, or (iii) to develop new export products.[6] While a single main objective had to be provided for each export business plan, firms could request assistance for other objectives. The export business plan was evaluated by a panel of five local and international experts and, if accepted, the FAMEX team would provide technical assistance to help the firm improve its plan. Then, the FAMEX team would draw up, together with the firm, a list of activities eligible for matching grants of up to 50 percent of their cost, with a ceiling of TND 100,000 (71,000 USD). Given that FAMEX operated under a reimbursement basis, whereby firms had to present receipts upon implementing their plan, we have reasonable confidence that the matching grant funds were used for their intended purpose.[7] Moreover, FAMEX beneficiaries were obliged to supply the FAMEX management team with data to allow an assessment of the project's impact on export growth.[8]

[3] Another broad-based component of the EDP project that we do not examine in this paper was a broad trade facilitation component which supported investments and technical assistance aimed at modernizing customs procedures through a combination of investment in hardware and software and procedural improvements. Such investments if effective benefited all Tunisian firms and thus will not affect our identification of FAMEX effects in the rest of the paper.

[4] One concern with an export promotion program such as FAMEX is its compatibility with WTO rules. The grants are actually considered as "non-actionable subsidies" (Article 8 of the Agreement on Subsidies and Countervailing Measures) because they were not specific to particular sectors nor particular export markets.

[5] The conversions from Tunisian dinars to USD are made based on the exchange rate as of October 10, 2010 (1 USD = 1.39 TND).

[6] While there was no clear rule on which firms were deemed to have little export experience, in interviews with the authors, FAMEX officers indicated that they included in that category firms that either did not export or exported an amount representing less than 20% of their turnover in the recent past.

[7] This feature of FAMEX makes it an attractive project to evaluate. The misuse of funds for purposes other than the intended purpose has been shown to be severe in World Bank-funded education projects by Reinikka and Svensson (2004).

[8] Supervision teams from the World Bank also had access to that information.

FAMEX received 1,710 applications and accepted 1,231 of those corresponding to 1,060 firms.[9] In terms of the main objective to request FAMEX assistance, nearly half of the beneficiaries (49%) were already exporters and wanted to expand into new destination markets; 31% had never exported, and 20% wanted to grow out of sole-buyer relationships. FAMEX program's coverage was fairly broad in terms of sectors and locations, as will be shown in Section 3.

FAMEX grants were used mostly to co-finance export marketing assistance in the form of consultant services and technical assistance. Specifically, the FAMEX program financed several types of activities: (i) market prospection, (ii) promotion, (iii) product development, (iv) firm development, and (v) foreign subsidiary creation. Market prospection activities relate to the acquisition of information and include for example the purchase of data or trade missions abroad to visit or participate in foreign exhibitions. Promotion activities relate to the production of information and marketing and include the design, production and publication of ads in various media, firm representation in fairs and exhibitions, and mailings. Product development activities involve the production of samples and package design. Firm development relates to organizational issues such as setting up a marketing watch, an export cell, or an export-oriented business plan. Finally, foreign subsidiary creation refers to assistance for the establishment of a facility abroad including legal, consulting, covering rental and salary costs for the first year of establishment. The amounts disbursed by FAMEX for each type of activity are shown in the first column Table 1. Note that grant shares in the second column add to 100%, but the number of firms in the third column adds up to more than the number of FAMEX beneficiaries because each firm typically received co-financing to undertake several activities.

In terms of actual disbursements, 25% of the FAMEX funds for the activities in Table 1 covered marketing research costs, 18% covered fees from private export-marketing consultants, 15% covered the participation in trade fairs, 10% went to establishing foreign representations, 10% covered printing costs for advertising material, and the rest was scattered over minor items.

Table 1
FAMEX Program Components

	Amounts disbursed (in million USD)	Share in program total	Number of firms
Market prospection	2.665	23.9%	313
Promotion	4.113	36.9%	319
Product development	1.515	13.6%	184
Firm development	1.169	10.5%	220
Foreign subsidiary creation	1.688	15.1%	84
Total	*11.150*	*100.0%*	

Note: Tunisian dinars were converted into U.S. dollars at the October 10, 2011 exchange rate (1.463 TND per USD).

[9] Some firms applied to FAMEX twice and had two export development plans accepted prior to 2009, some firms started a second export development plan in 2009, and some firms dropped one export development plan before re-applying to FAMEX.

3. Data and Descriptive Statistics

3.1 Combined Dataset

To evaluate the impact of the FAMEX program we need to obtain data on FAMEX beneficiary firms as well as on a control group of non-beneficiary firms. Our dataset combines three main sources of data: (1) FAMEX program data, (2) data from the National Statistical Institute (INS in its French acronym) and the investment-promotion agency (API in its French acronym), and (3) customs exporter-level data. Next, we provide more details on each of the sources of data.

First, we obtained from the FAMEX management the complete list of the 1,060 FAMEX beneficiary firms indexed by their tax ID. After dropping (i) 310 firms for whom the impact of FAMEX could not be assessed since they had their first export development plan still ongoing at the end of 2009, (ii) 120 firms which dropped out of the FAMEX program, and (iii) 175 firms in the services sector for which customs export data is not available, we are left with 455 FAMEX beneficiaries. [10] For these beneficiaries we obtained FAMEX program data covering the following variables: the years when the firm joined and terminated the program (the years vary across firms), firm location, sector, number of employees and turnover when it joined the program and when it left it, whether the firm had an in-house export unit prior to joining the program, , the firm’s objective in applying to FAMEX, the grant use (total disbursement and breakdown), and a grade (A, B or C) assigned ex-post by FAMEX to its overall performance under the program.

Second, we requested from the National Statistical Institute (INS in its French acronym) a stratified sample of control firms with a structure similar to that of the 455 FAMEX beneficiaries. The stratification was performed based on size, prior exporting status, and sector (within manufacturing), resulting in 48 cells. For each cell we asked INS to provide us with roughly twice as many non-beneficiaries as there were FAMEX beneficiaries. To draw our stratified sample of control firms INS used its 2007 census of firms that includes information on firm location, sector, number of employees, turnover by stratum, and date of firm creation.[11] From the INS census, we obtained a group of 910 control firms. Since the INS data was incomplete for a number of firms, we supplemented it with data obtained from Tunisia’s investment-promotion agency (API in its French acronym). The API database includes employment, sector, date of firm creation, social capital, and offshore status for 5,000 firms (of which 500 are also in the INS database). From the API database, we extracted a group of 2,000 control firms that were neither in the FAMEX sample nor in the INS sample. Note that all control firms are exporting firms given that stratification was based on prior exporting status.

[10] Dropout firms are those that dropped out of FAMEX and did not apply for a second FAMEX plan.

[11] The turnover strata were: less than TND 50,000, TND 50,000-1 million, TND 1 million-2 million, TND 2 million-5 million.

Using exporters only in both the treatment and control groups makes the overall sample more homogeneous and therefore improves the estimation.[12]

Third, we obtained from the Tunisian Customs authority transaction-level export data for the 455 FAMEX beneficiaries, the 910 control firms from INS, and the 2,000 control firms from API. For each firm we obtained the tax ID, monthly transaction value, country of destination, product code (using an 11-digit Tunisian nomenclature derived from the Harmonized System), and weight, for every year between 2000 and 2010. Transaction-level export data was aggregated up to annual totals.

We combine data from the three sources and after checks for data consistency and cleaning procedures, we obtain our combined dataset which is an unbalanced panel of yearly export activity for 2,746 exporting firms with an average of 6 years of data per firm.[13] Of these firms, 401 benefitted from FAMEX and 2,346 did not. Among the 2,346 non-beneficiaries, 71 were firms who applied to the FAMEX program but were rejected while 126 were firms whose applications to the FAMEX program were accepted but who dropped out of the program for varied reasons. The set of 2,346 firms constitutes our control group in the baseline specifications. In robustness specifications we will experiment with including the 126 FAMEX dropouts in the treatment group instead of the control group while in others we will drop the 126 FAMEX dropouts from the sample altogether. In the combined dataset, firm-level characteristics other than those related to export transactions are time-invariant, being available only for 2007. As mentioned in Section 1, the advantage of being able to merge customs transaction data with other sources of firm-level data is that with customs data there is no recall bias in the outcome variables.

3.2 Descriptive Statistics and Stylized facts

Table 2 provides descriptive statistics for FAMEX beneficiary and non-beneficiary firms in the sample in terms of their sector, location, employment and sales categories. The sectoral distributions of FAMEX treated firms and control firms are quite similar, with the exception of textiles and apparel is particularly highly represented among control firms. That sector is also the major sector for FAMEX firms, it accounts for 31% of the firms. Although location was not used for stratification, the distributions of FAMEX firms and control firms across locations are not too different: FAMEX firms are more concentrated in Tunis while control firms are more concentrated in the rest of Tunisia. Only minor differences are shown for the distributions of

[12] By “exporters”, we mean firms having a customs code and having conducted at least one export transaction over the sample period.

[13] We should note that the merging of the three data sources was possible thanks to the use of unique tax IDs by all the Tunisian administrations concerned and to their willingness to share the data with us. Some of the data inconsistencies addressed were wrong sectoral classifications in the FAMEX program data that had to be corrected using INS and API data.

FAMEX firms and control firms across size categories measured in terms of employment or sales.

Table 2
Summary Statistics

(a) Distribution by Sector

Sector	Agro-industry (%)	Textile & apparels (%)	Paper wood furniture (%)	Chemicals (%)	Metals (%)	Machine & equipment (%)	Electric (%)	Total number of firms
FAMEX firms	15	31	13	12	8	14	6	401
Control firms	11	43	9	11	7	11	7	2346

(b) Distribution by Region

Location	Tunis	Grand Tunis	Central Sea	Rest of Tunisia	Total number of firms
FAMEX firms	22	48	28	2	401
Control firms	10	46	37	8	2346

(c) Distribution by Employment Category

Employment	[1,9]	[10,19]	[20,49]	[50,99]	[100,199]	>=200	Total number of firms
FAMEX firms	11	9	29	19	16	16	401
Control firms	5	12	31	23	17	12	2346

Source: Authors' calculations using the combined dataset.

Table 3 shows the export trends between 2000 and 2010 for FAMEX beneficiaries and non-beneficiaries, as well as for Tunisian manufacturing exporters as a whole. There is no prima-facie evidence of superior performance for FAMEX firms. In fact, considering the sample period as a whole they perform on average worse than non-beneficiaries and both groups perform substantially worse than the universe of Tunisian manufacturing exporters. Moreover, in the early stage of the global financial crisis in 2007-2008, FAMEX firms recorded a 6% drop in total exports while other firms still had positive export growth. In the recovery phase FAMEX firms are growing at a smaller pace (2%) than their counterparts that did not receive assistance. This might be indicative of risk-taking behavior by treated firms: exposing themselves to destination markets that stood to contract most at the outset of the crisis and that experienced a slower recovery. We will return to this conjecture later in the paper. Also note that our sample of FAMEX and control firms accounts for an important share of total Tunisian exports.

Table 3
Growth in Tunisia’s exports: FAMEX Beneficiaries, Non-Beneficiaries, and Total

	2003-2004	2004-2005	2005-2006	2006-2007	2007-2008	2008-2009	2009-2010	2003-2010
Growth in total exports of:								
FAMEX firms	16%	27%	3%	12%	-6%	-12%	2%	42%
Control firms	24%	6%	7%	18%	3%	-16%	4%	51%
Tunisia	21%	8%	13%	25%	21%	-21%	8%	95%
Share of exports by FAMEX and control firms in Tunisia total exports	59%	60%	61%	57%	50%	49%	55%	53%

Source: Authors’ calculations using the combined dataset and data from COMTRADE.
Note: The row ‘Tunisia Total’ shows the growth in exports for the country as a whole excluding phosphates based on COMTRADE data.

4. Estimation issues

To understand whether the FAMEX intervention was effective in its objective of promoting export competitiveness in Tunisia we need to estimate the causal effect of FAMEX on firm-level export outcomes. The main identification problem in this evaluation is that the assignment to FAMEX assistance is far from random. In particular, FAMEX beneficiaries may differ substantially from FAMEX non-beneficiaries in characteristics that affect both their participation as well as the export outcomes. Hence it is vital to use non-experimental impact evaluation methods which address that selection bias and obtain a credible estimate for the counterfactual of what would have happened to FAMEX beneficiaries if they had not received assistance.[14]

As a first approach to identify the impact of FAMEX, we consider a propensity score matching difference-in-differences (PSM-DID) estimator. This estimator has been widely used in evaluation of programs in several areas – including the area of export promotion by Görg, Henry and Strobl (2008) and Volpe and Carballo (2008).[15] The PSM-DID method controls for selection bias by comparing the change in outcomes for program beneficiaries relative to the change in outcomes for “observationally similar” control firms before and after the program.[16] In our study, “observationally similar” firms will be defined based on a propensity score which is the probability that a firm receives FAMEX assistance, given a rich set of observable firm covariates, and on a metric of proximity between propensity scores. The PSM-DID estimator is based on the twin assumptions that (i) assignment to treatment (or the decision to undertake it) is

[14] This is the fundamental problem of causal inference defined by Holland (1986).
[15] A seminal study employing this method for program evaluation is Heckman, Ichimura, and Todd (1997). See Heinrich, Maffioli, and Vazquez (2010) for other examples of studies using PSM-DID as well as for a practical guide on how to implement the technique.
[16] The rationale underlying the PSM-DID method is the idea of reproducing the treatment group among the control group and thus of reestablishing “the experimental conditions in a non-experimental setting” (Blundell and Costa Dias, 2009). The matching assumptions ensure that the only remaining difference between the two groups is program participation.

independent of potential outcomes, conditional on observed pre-treatment covariates; and (ii) there is sufficient overlap in the distribution of propensity scores between the treatment and control groups (i.e., it is possible to find matches for all or most treated firms).[17] While the PSM-DID estimator is based on the selection on observables assumption (i), by relying on a comparison of changes in outcomes, it controls for time-invariant unobserved pre-program differences across firms potentially leading to self-selection into the program and influencing outcomes (Blundell and Costa Dias, 2009).

Let y_{it} be an export outcome variable with i indexing firms and t years, T be the treatment group, D_{it} be the treatment indicator variable, and $t(i)$ the time at which firm i enrolls into FAMEX. Thus,

$$D_{it} = \begin{cases} 1 & \text{if } i \in T \text{ and } t \geq t(i) \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

Suppose first for simplicity that all firms undergo treatment, i.e., receive FAMEX export marketing assistance, in the same year τ. Let T, C and S be respectively the treatment and control groups and their common support. Let also $y_{i,\tau-1}$ and $y_{i\tau}$ be firm i's outcome before and after treatment, and. The PSM-DID estimator is given by:

$$\hat{\gamma}^{PSM-DID} = \sum_{i \in T \cap S} \left[\Delta y_i - \sum_{j \in C \cap S} w_{ij} \Delta y_j \right] \tag{2}$$

where

$$\Delta y_i = y_{i\tau} - y_{i,\tau-1} \tag{3}$$

and the weights w_{ij} are determined by the matching method.[18]

While we will show PSM-DID estimates in Section 5, a complication arises in our setup as the treatment year is not the same for all firms: some joined FAMEX in 2005, others in subsequent years up to 2009. Let $t(i)$ be the year of enrollment into FAMEX or treatment year for firm i. The expression for the before-after difference in outcomes is thus:

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[17] Assumption (i) - often designated as selection on observables - indicates that assignment to treatment is unconfounded or independent of outcomes conditional on covariates (Hirano, Imbens, and Ridder, 2003).

[18] The control group $C \cap S$ is formed by picking for each treated firm the control firms with closest propensity score. Depending on the matching method there can be for each treated firm either one matched control firm or several, using a weighted scheme. Nearest-neighbor matching identifies the firm in the control group with the closest propensity score to that of treated firm i. Because the distance is unrestricted, it can be large. Caliper maching solves this problem by imposing a predefined maximum distance. If no control-group firm is found within that distance, treated firm i is ignored. Nearest-neighbor matching can be extended to any fixed number of matches. It also has two variants: with replacement (where the same control firm can be used for several treated firms if it happens to be their common nearest-neighbor) or without replacement (where it can be used only once). An alternative method is kernel matching, which uses a weighted average of *all* control-group firms as a match for *each* treated firm i (see Heckman, Ichimura and Todd, 1997). See Caliendo and Kopeinig (2008) for additional details on matching.

$$\Delta y_i = y_{i,t(i)} - y_{i,t(i)-1} \tag{4}$$

instead of Eq. (3). This expression is well defined for treated firms. However, it is not defined for untreated firms for which a treatment year is not available. As a result, in standard statistical packages for propensity score matching estimation (such as psmatch2 in Stata) the years when treatment firms are matched with control firms are disregarded, which may be problematic if calendar time matters for performance.

One approach to deal with this issue is to obtain a PSM-DID estimator that restricts control firms matched to treated firm *i* to be taken in *t*(*i*).[19] The PSM-DID estimator then becomes:

$$\hat{\gamma}^{PSM-DID'} = \sum_{i \in T \cap S} \left[\left(y_{i,t(i)} - y_{i,t(i)-1} \right) - \sum_{j \in C \cap S} w_{ij} \left(y_{j,t(i)} - y_{j,t(i)-1} \right) \right] \tag{5}$$

We will show results for the estimator in Eq. (5) in our robustness analysis in Section 7.

An alternative approach consists of using a weighted-least squares (WLS) regression following Hirano, Imbens, and Ridder (2003) where weights are a function of the estimated propensity scores. Those weights can create a balance in covariates across treated and control firms and hence allow for an unbiased regression-based estimator of the effect of a program.[20] This will be our second and main approach to estimate the impact of FAMEX.

Let $\hat{p}_i$ be the estimated propensity score of firm *i* and $\hat{r}_i = \hat{p}_i / \left(1 - \hat{p}_i\right)$ its estimated odds ratio. Hirano, Imbens, and Ridder (2003) propose to estimate a weighted least squares regression of an outcome on an indicator for treatment and other covariates giving a weight of 1 to observations of treated firms and a weight of $\hat{r}_i$ for observations of control firms. The advantage of using a regression framework is the possibility of controlling for covariates or firm fixed effects, if the covariates are time-invariant. Since our combined dataset is a panel of firms over the 2004-2010 period, a regression framework is attractive in that it allows to control for cyclical trends through year fixed effects. Hirano, Imbens, and Ridder (2003) show that the use of such WLS regressions results in an estimator that is more efficient than the PSM-DID estimator. Our baseline regression equation, to be estimated by WLS, explains changes in firm outcomes and can thus be thought of as a difference-in differences (DID) equation:

$$\Delta y_{it} = \alpha + \beta I_{it} + \overline{X}_i \gamma + \delta_t + u_{it} \tag{6}$$

[19] Todo (2011) uses PSM-DID estimators for the evaluation of the impact of Japanese aid-funded technical assistance programs on Indonesian foundry firms based on propensity score matching done year by year. We are grateful to Yasusuki Todo for sharing his matching routine. The routine restricts treated firm *i*'s matches to be exclusively control firms taken at *t(i)*. This substantially reduces the number of possible matches and is therefore feasible only with a sufficiently large initial control group.

[20] See DiNardo, Fortin and Lemieux (1996), Van de Walle and Mu (2007) for applications of the method and Morgan and Harding (2006) for an overview. Hirano, Imbens, and Ridder (2003) show that the use of such WLS regressions results in an estimator that is more efficient than the PSM-DID estimator.

where $\overline{X}_i$ is a vector of time-invariant firm covariates and the indicator variable for FAMEX participation is defined as:

$$I_{it} = \begin{cases} 1 & \text{if } i \in T \text{ and } t = t(i) \\ 0 & \text{otherwise.} \end{cases} \tag{7}$$

One concern with the definition in Eq. (7) is that it allows treated firms after their year of treatment to be part of the control group. To address this concern we set I_{it} to a missing value for $t > t(i)$. We also set I_{it} to a missing value for all $t < t(i) - 1$. Thus, in the baseline regression we consider only the treatment year and the year just prior to treatment which means that we are left with a two-period panel for each firm. Moreover, we take first differences of the outcome variable as in Eq. (4). Thus, the sample structure degenerates into a cross-section of firms in which calendar time comes in only through the year of enrolment into FAMEX. We control for that through time effects $\delta_{t(i)}$ which indicate whether firm i enrolled in FAMEX in 2005, 2006, or any subsequent year up to 2009. The baseline WLS regression equation is thus given by:

$$\Delta y_i = \alpha + \beta I_i + \overline{X}_i \gamma + \delta_{t(i)} + u_{it} \, . \tag{8}$$

Note that our WLS regressions will be estimated for the treated and control firms that are part of the common support identified by the propensity score matching. We will explore variants of Eq. (8) in which we consider one-year differences in outcomes in the rest of period after the enrollment into FAMEX and other variants in which we consider longer time differences relative to the year of enrollment.

While Glazerman, Levy, and Myers (2003) show that the PSM method can reduce bias in program impacts particularly when combined with DID or with WLS regression, as in our case, and the use of such methods is now pervasive in the evaluation of public programs, these estimators have limitations. In particular, the resulting estimates may be biased if unobserved time-varying firm characteristics affect both participation and outcomes. In a non-experimental study such as ours, selection bias on unobservables can never by fully ruled out.

5. Main Results

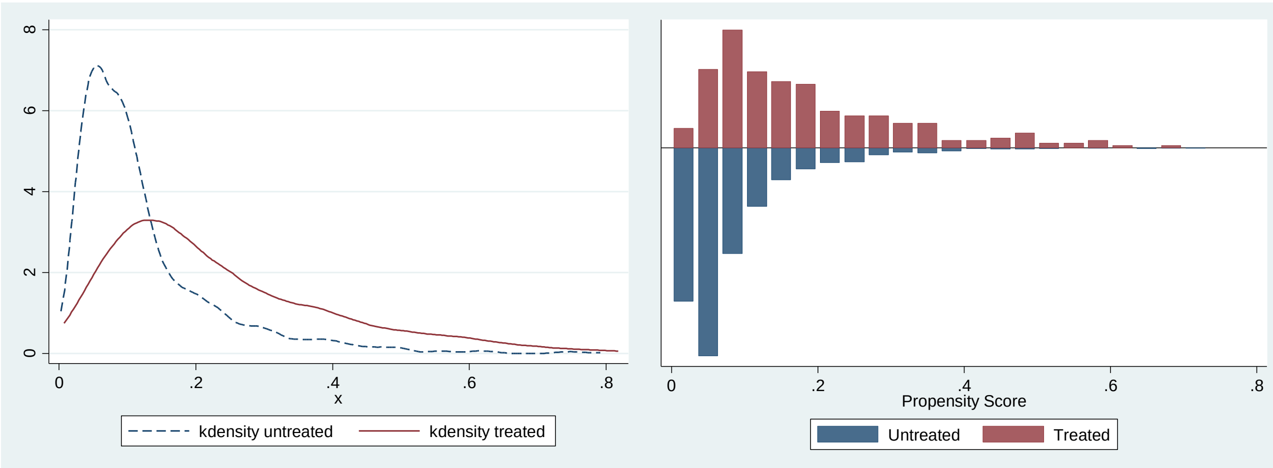
5.1 Matching

A key ingredient for both the PSM-DID estimator and our WLS regression estimators are propensity scores. These are estimated by a cross-sectional probit regression on the full sample explaining the probability of Tunisian firms to receive a FAMEX grant in any year between 2005 and 2009.[21] We consider a rich set of firm covariates as explanatory variables: firm age in levels and squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial pre-FAMEX total exports, number of exported products and number of destinations served.[22] Appendix Table A.1 presents the probit regression results. The estimates show that firms with larger exports and those exporting 100% of their output in 2004 are significantly less likely to receive a FAMEX grant thereafter, whereas smaller exporters and those exporting more products and shipping to more destinations are more likely. Exporters located in Tunis are significantly more likely to receive a FAMEX grant. The insignificance of the sector fixed effects suggests that FAMEX grants did not target particular sectors. Propensity scores for each Tunisian firm are retrieved from the probit regression as the predicted probabilities of getting treated and their densities for treated and control groups are shown in Figure 1. The relatively similar shapes of the densities and the seemingly very large common support imply that most FAMEX firms can, in principle, be matched to one or more control firms based on the closeness of propensity scores.[23] However, the imperfect overlap in the densities also suggests that it is important to use matching and re-weighting to select control firms that are more comparable to FAMEX firms. We perform propensity score matching using a kernel estimator, which is a non-parametric estimator that uses a weighted average of all control firms to match each treated firm, where the weights depend on the distance between the propensity score of a control firm and the treated firm.

Imposing the common support condition for this matching (as per assumption (ii) above for PSM-DID estimators) would imply dropping treated firms whose propensity score is higher than the maximum or lower than the minimum score for the control group. In our case, this results in no loss of treated firms. Our common support includes all 401 FAMEX beneficiaries and 2,346 control firms. To assess the quality of the propensity score matching we implement tests for the balancing hypothesis proposed by Rosenbaum and Rubin (1985), Dehejia and Wahba (2002) and Smith and Todd (2005). The rationale behind the tests is to assess whether the matching is able to balance the distribution of the covariates in the treatment and control groups (Caliendo and Kopeining, 2008). The details on the tests are presented in the Appendix. Overall, the results from the balancing tests suggest that our matching procedure is able to generate sufficiently “similar” control firms to match to each treated firm in the common support.

[21] While we use the customs data for 2009 and 2010, we exclude from the sample the firms that received the FAMEX grant in 2009 since we are unable to assess the effect of the grant for such firms.
[22] We use data for 2004 for the initial export-related variables. As Bernard et al. (2010) show, more productive firms tend to export more products to more destinations, therefore controlling for the initial number of products and destinations is a way of controlling indirectly for TFP, for which we have no data. On this, see the discussion in Footnote 24 of Volpe and Carballo (2008).
[23] The importance of a large common support and of similarity in the distribution of covariates or propensity scores across treated and untreated groups for unbiased matching DID estimators is shown by Heckman, Ishimura and Todd (1997).

Figure 1
Densities and histogram of propensity scores, treatment and control groups



Source: Authors' calculations.

5.2 Baseline Results

Table 4 presents results from a PSM-DID estimator as well as from WLS regressions using the weighting scheme based on propensity described in the previous section for three alternative firm-level outcome or performance variables: total exports (in Tunisian dinars), number of destinations, and number of exported products, all in log-differences. Column (1a) shows the PSM-DID estimates of FAMEX in the year of enrolment based on Eq. (5) with w_{ij} being determined by the kernel matching method to serve as a benchmark.[24] Column (1b) shows the estimated impact of receiving FAMEX assistance in the year of enrolment (designated below and in the table as "TY") based on the WLS regression in Eq. (8). In columns (2)-(6) we estimate the impact of FAMEX in years subsequent to enrolment by modifying the dependent variable in Eq. (8) to be defined as:

$$\Delta y_i^{TY+k} = y_{i,TY(i)+k} - y_{i,TY(i)+k-1} \quad (9)$$

for k ranging from 1 to 5. To further clarify the estimates shown in Table 3, consider the example of a firm enrolled in FAMEX in 2005 and the outcome total exports. Regarding the dependent variable of Eq. (8), it is the change in log-total exports between 2004 and 2005 in columns (1a) and (1b); the change in log-total exports between 2005 and 2006 in column (2); the change in log-total exports between 2006 and 2007 in column (3); and so on. Regarding the indicator

[24] Note that the standard errors reported for the PSM-DID estimator are based on the estimator of the asymptotic variance, as bootstrapped standard errors can be invalid for this estimator because its asymptotic properties are not known (Abadie and Imbens, 2006). The problem is particularly severe for matching methods relying on a small number of controls (nearest neighbor or n-nearest neighbors). However, we obtain qualitatively similar findings when using bootstrapped standard errors.

variable for treatment to include in Eq. (8), it is 0 until 2004, 1 in 2005, and missing thereafter in columns (1a) and (1b); it is 0 until 2005, 1 in 2006, and missing thereafter in column (2); it is 0 until 2006, 1 in 2007, and missing thereafter in column (3); and so on. This definition of the indicator variable ensures that we never allow treated firms to be included in the control group in the years post-treatment (which would happen if the treatment indicator variable switched back to zero after enrolment). To estimate the specifications in Table 4 we pool across all Tunisian firms receiving the FAMEX treatment in different years and all controls in the common support. All regressions use robust White-corrected *t*-statistics.

Table 4
Year-to-Year Effects of FAMEX on Export Outcomes

Difference	TY-(TY-1)	TY-(TY-1)	(TY+1)-TY	(TY+2)-(TY+1)	(TY+3)-(TY+2)	(TY+4)-(TY+3)	(TY+5)-(TY+4)
Estimator	PSM-DID (1a)	WLS reg. (1b)	WLS reg. (2)	WLS reg. (3)	WLS reg. (4)	WLS reg. (5)	WLS reg. (6)
<u>Outcome</u>							
Total exports	0.562	0.511	0.251	-0.042	-0.157	-0.240	0.025
	(2.66)**	(3.08)***	(1.55)	(-0.26)	(-0.83)	(-1.06)	(0.11)
R-squared		*0.17*	*0.14*	*0.11*	*0.09*	*0.11*	*0.11*
Nb. destinations	0.113	0.150	0.086	0.052	0.021	0.036	0.059
	(4.33)**	(6.10)***	(3.70)***	(2.10)**	(0.84)	(1.11)	(2.07)**
R-squared		*0.15*	*0.12*	*0.08*	*0.12*	*0.12*	*0.08*
Nb. products	0.11	0.147	0.071	0.049	0.008	0.060	0.097
	(5.59)***	(4.68)***	(2.22)**	(1.59)	(0.23)	(1.59)	(2.58)***
R-squared		*0.15*	*0.13*	*0.13*	*0.12*	*0.13*	*0.13*
Observations		12,263	12,214	9,803	7,401	4,975	2,607

Notes: T-statistics based on robust standard errors in parentheses; *: significant at 10%; **: significant at 5%; ***: significant at 1%. The sample includes treated firms and control firms in the common support. PSM-DID estimates and WLS regressions are estimated based on propensity scores obtained using kernel matching. See the text for the definition of the outcome variables across columns. The WLS regressions include treatment year fixed effects and the following firm covariates: firm age and age squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial total exports, number of exported products and number of destinations served.

Several observations stand out from Table 4. Impact effects in columns (1a) and (1b) are significant at the 1% level for all three outcome variables. The results from WLS regressions in column (1b) are in fact very close to those obtained by PSM-DID in column (1a). The magnitudes are large: 50% more growth in total exports (measured in logarithms of values) and 15% more growth in export destinations and products (measured in logarithms of counts) for FAMEX beneficiaries, compared to the control group. However, growth effects vanish after the treatment year, remaining significant in the second year only in the case of the number of destinations. Oddly, destination and product growth show positive and significant effects five years after treatment. This estimate is based only on firms that enrolled in FAMEX in 2005, and it means that they enjoyed in 2010 a resumption in destination and product growth, compared to control firms. The sample size used for the WLS regressions shrinks when moving from column (2) to column (6) in Table 4. For example for a firm enrolled in FAMEX in 2008, the observations to compute its TY*(i)*+3- TY*(i)*+2 difference are not available. Hence censoring progressively reduces the sample by eliminating firms that enrolled in FAMEX in later years. Since the WLS regression allows us to condition on the year of treatment through year fixed effects, this is not a major problem. However, for robustness purposes we re-estimated all the specifications in Table 4 on a restricted sample including only firms enrolled in FAMEX in 2005 and control firms in the common support. The unreported results are qualitatively very similar to those in Table 4.[25] While the results in Table 4 are based on matching using a kernel estimator we obtain qualitatively similar results using a five-nearest neighbours estimator as well as a radius estimator with caliper equal to 0.1.[26]

In Table 5, we take a different angle on persistence and report cumulative effects of FAMEX, which show for how long the impact effect remains perceptible. Thus, we modify the dependent variable in Eq. (8) to be defined as:

$$\Delta y_i^{TY+k} = y_{i,TY(i)+k} - y_{i,TY(i)-1} \tag{10}$$

for k ranging from 1 to 5. Thus, the log-difference is taken over increasingly long time intervals as k rises, always relative to the year before FAMEX treatment. To estimate the specifications in Table 4 we pool across all Tunisian firms receiving the FAMEX treatment in different years and all controls in the common support.

The effects on total exports in Table 5 do not vanish one year after the enrolment in FAMEX because the positive impact effect of the treatment year carries over. But after three years, again no effect remains on total exports, as shown by columns (4)-(6). Table 5 shows, however, long-lasting positive effects on destinations and products of FAMEX beneficiaries relative to control firms. Given the sample shrinkage that is also observed across columns in Table 5 we also re-estimated all specifications on a restricted sample including only firms enrolled in FAMEX in 2005 and control firms in the common support and found qualitatively similar results.[27]

To sum up, according to Table 4, after one year after FAMEX enrolment, total exports' growth rates stop differing between the treatment and the control groups; according to Table 5, after three years, even total exports' levels have converged back to similar levels, as the cumulative log-change is no longer significantly different across FAMEX beneficiaries and control firms. Figure 1 summarizes the findings. Whatever the econometric approach, we find the same patterns: FAMEX had a positive impact on export performance in the year of treatment, while considering a longer time period of up to five years post-treatment, FAMEX had positive impacts only on the extensive margins of trade in terms of products and destinations.

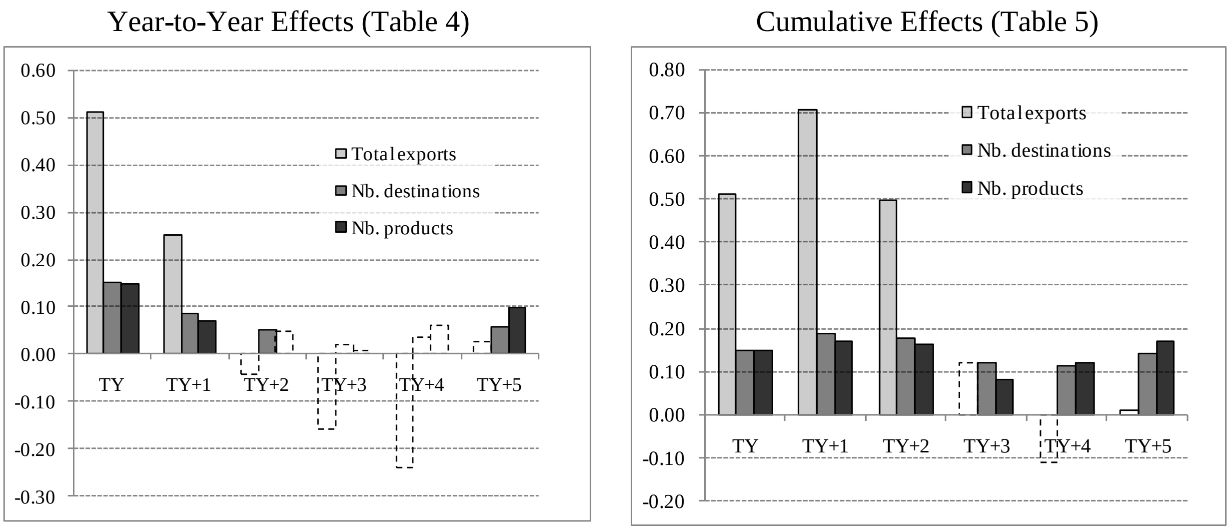
[25] The results are available upon request. We are grateful to Beata Javorcik for attracting our attention to this issue.
[26] The results are available upon request.
[27] The results are available upon request.

Table 5
Cumulative Effects of FAMEX on Export Outcomes

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.511	0.707	0.499	0.120	-0.113	0.008
	(3.08)***	(3.53)***	(2.27)**	(0.45)	(-0.34)	(0.02)
R-squared	*0.17*	*0.22*	*0.22*	*0.22*	*0.23*	*0.25*
Nb. destinations	0.150	0.187	0.178	0.122	0.113	0.141
	(6.10)***	(6.81)***	(5.60)***	(3.43)***	(2.45)**	(2.65)***
R-squared	*0.15*	*0.19*	*0.20*	*0.22*	*0.27*	*0.28*
Nb. products	0.147	0.171	0.163	0.081	0.119	0.170
	(4.68)***	(4.62)***	(4.09)***	(1.77)*	(2.06)**	(2.67)***
R-squared	*0.15*	*0.19*	*0.23*	*0.25*	*0.25*	*0.28*
Observations	12,263	12,263	9,915	7,526	5,087	2,656

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. The sample includes treated firms and control firms in the common support. See the text for the definition of the outcome differences across columns. The WLS regressions include treatment year fixed effects and the following firm covariates: firm age and age squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial total exports, number of exported products and number of destinations served.

Figure 2
Year-to-Year and Cumulative Effects



Note: Bar heights show the point estimates of the coefficients in Table 4 and Table 5. Insignificant effects are shown as empty bars.

5.3 Heterogeneity based on Project Characteristics

Ravallion (2009, p. 37) argued that practitioners should "never be happy with an evaluation that assumes common (homogeneous) impact". Given the level of detail available in our FAMEX program data, we are able to investigate more thoroughly the effect of FAMEX by exploiting two dimensions potentially affecting the treatment effect: the objective of the individual project that FAMEX was supporting and the specific activities supported by the FAMEX grant. To our knowledge, this type of analysis is novel in the export-promotion literature.

Tunisian firms had to state an objective when applying for FAMEX assistance, as mentioned in Section 2: whether they wanted (i) to become a significant exporter, (ii) to export to a new destination, or (iii) to export a new product. Given the way in which FAMEX application packages were structured, firms could state only *one* of these three objectives, hence the objectives partition the treatment group into three non-overlapping "bins". Accordingly, we re-estimate Eq. (8) to obtain the effect in the year of treatment (as in column (1b) of Table 4) but allowing this effect to differ across bins and present the results in

Table 6. The distribution of the 401 treated firms across objectives is as follows: 95 came to FAMEX to start exporting, 194 came to FAMEX to export to a new market, and 112 came to FAMEX to export a new product.

Table 6
Effect of Treatment Interacted with Project Objective

Difference	TY-(TY-1)	TY-(TY-1)	TY-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.
Outcome	Total exports (1)	Nb. destinations (2)	Nb. products (3)
Nature of project			
Start exporting	0.467	0.144	0.130
	(1.15)	(2.66)***	(1.89)*
New destinations	0.563	0.171	0.156
	(2.48)**	(4.95)***	(3.43)***
New products	0.184	0.085	0.082
	(0.78)	(2.42)**	(1.72)*
R-squared	*0.17*	*0.16*	*0.15*
Observations	12,263	12,263	12,263

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The WLS regressions include treatment year fixed effects and the following firm covariates: firm age and age squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial total exports, number of exported products and number of destinations served.

In principle, one would expect stronger results for the coefficients on the "diagonal", i.e., on total export growth for firms wanting to start exporting (column 1), on destination growth for those looking for new markets (column 2), and on product growth for those wanting to export new products (column 3). However, the evidence is not supportive of that conjecture. Instead, the estimates suggest that, for all outcome variables, FAMEX firms whose objective was to export to new markets exhibit the strongest treatment effects while those whose objective was to export new products exhibit the lowest treatment effects.

The FAMEX program financed several types of activities as described in Section 3: (i) market prospection, (ii) promotion, (iii) product development, (iv) firm development, and (v) foreign subsidiary creation. To analyze the effect of the FAMEX program's components, we replace the binary treatment effect used in Section 5.2 with a vector of continuous variables measuring, for each firm, the amount of FAMEX funding earmarked under each specific type of activity (the equivalent of the first column of Table 1, but at the firm level) which is available for 328 FAMEX beneficiaries.[28] At this stage, we control for selection into the program through the usual propensity score matching weighting scheme, but we do not control for selection into particular levels of support for each activity. We will turn to a formal analysis of the effect of grant size in the next section. The results from re-estimating Eq. (8) to obtain the effect in the year of treatment (as in column (1b) of Table 4) including the vector of amounts spent per activity are shown in Table 7.

Market prospection activities have a significant impact effect on all outcome variables. For instance, a thousand Tunisian dinars spent on prospection raise total export growth of FAMEX beneficiaries by 4.2 percentage points (recall that the dependent variable is a log-difference, whereas the amounts are entered linearly). Promotion activities also have a positive and significant impact on the three outcomes, with relatively similar marginal returns on the dinar. By contrast, the other three types of activities have insignificant returns. One rationale for this absence of effects could be that when highly specialized skills are required for an activity such as fashion packaging (within product development), additional co-financing by FAMEX does not change the overall picture: either the firm has the right skilled staff or outsources the task to the right firm, or it has not. In the latter case FAMEX support does not improve export-related outcomes. Another rationale could be that the FAMEX administration was bad at identifying the right partners for product development and at advising on organizational issues, which require management experience that bureaucrats typically lack.

[28] The main reason for using the vector of amounts instead of bins for the different types of activities is to avoid multicollinearity across the individual components of the treatment that would arise due to their large overlap. Note that for each activity the amount entering in the WLS regressions is the amount co-financed at 50% by FAMEX. The total amount spent by the firm in that activity is actually twice as large.

Table 7
Effects of FAMEX Program Components

Difference	TY-(TY-1)	TY-(TY-1)	TY-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.
Outcome	Total exports	Nb. destinations	Nb. products
	(1)	(2)	(3)
Activity (amounts in TND)			
Market prospection	0.039	0.007	0.009
	(2.03)**	(2.07)**	(2.05)**
Promotion	0.028	0.006	0.004
	(3.06)***	(3.20)***	(1.11)
Product development	-0.014	0.000	-0.003
	(-0.96)	(0.06)	(-1.39)
Firm development	-0.022	0.001	-0.002
	(1.12)	(0.27)	(-0.41)
Foreign subsidiary creation	-0.003	-0.000	0.002
	(-0.15)	(-0.00)	(0.49)
R-squared	*0.205*	*0.168*	*0.156*
Observations	12,187	12,187	12,187

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The WLS regressions include treatment year fixed effects and the following firm covariates: firm age and age squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial total exports, number of exported products and number of destinations served. The vector of amounts used is in Tunisian dinars.

5.3 Treatment Intensity: Does Money Matter?

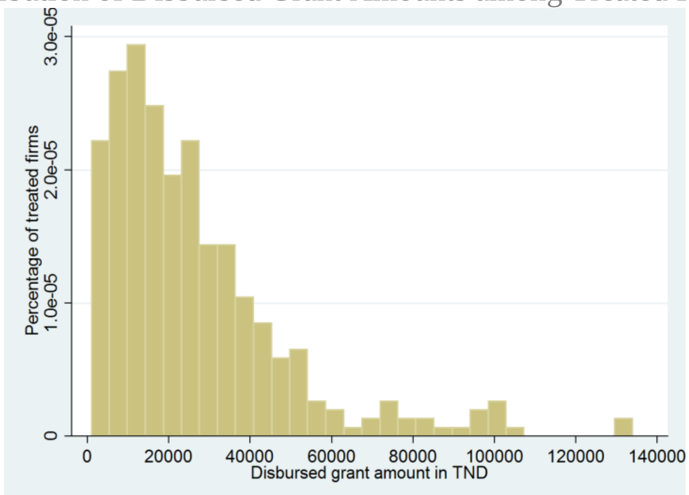
Given the matching-grant nature of the FAMEX program it is of great interest to explore whether treatment effects differ systematically as a function of the grant amounts disbursed. We use the Generalized Propensity Score (GPS) methodology developed by Imbens (2000) and Hirano and Imbens (2004), which makes it possible to construct a "dose-response" function showing the estimated causal effect of all "dosages" of the treatment on the various export-related outcomes.[29] The different amounts disbursed by FAMEX across firms constitute different "dosages" of the treatment. We focus on the impact in the year of treatment (as in column (1b) of Table 4).

The implementation of the GPS methodology involves three stages, described in Fryges (2008), Fryges and Wagner (2008), and Bia, Flores and Mattei (2011).[30] The first stage estimates the conditional density of the treatment variable given observable firm covariates or GPS. In our case, grants are officially capped at TND 100,000. The distribution of treated firms across grant

[29] In their study on the effectiveness of subsidies in promoting exports in Ireland, Görg, Henry and Strobl (2008) use information on grant amounts per plant. However, their approach suffers from limitations in the sense that they group grant amounts into a small number of categories instead of using the GPS methodology.
[30] We follow Fryges' treatment of the GPS method, with thanks for the do-files that he kindly shared with us.

amounts is shown in Figure 3. Out of the 345 treated firms for which information on grants disbursed is available, 5 obtained grants in excess of this ceiling (the highest being TND 134,000) and will be excluded from the analysis below.

Figure 3
Distribution of Disbursed Grant Amounts among Treated Firms



Note: density is based on 345 FAMEX beneficiaries for which disbursed grant amounts are available.

In what follows, we consider the treatment variable to be distributed between zero and TND 100,000. We express treatment "dosage" or treatment "intensity" as a continuous variable defined on the unit interval:

$$D_i(\beta) = z_i / 100{,}000 \quad \in [0,1] \tag{11}$$

where z_i is the size of the grant received by firm i in Tunisian Dinars. The distribution of the treatment intensity over the entire sample (treatment and control) has a large mass at zero corresponding to the control group's observations and cannot be considered normal even after algebraic transformation (Bia and Mattei, 2008). Accordingly, we assume that the treatment intensity follows a logistic distribution and we estimate a special case of the fractional-logit model of Papke and Wooldridge (1996), which consists in maximizing the Bernoulli likelihood function:

$$L_i(\beta) = \left[\Lambda\left(\overline{X}_i\beta\right)\right]^{Di}\left[1 - \Lambda\left(\overline{X}_i\beta\right)\right]^{1-D_i} \tag{12}$$

where $\Lambda(.)$ is the logistic distribution's cumulative distribution function, i.e., $\Lambda\left(\overline{X}_i\beta\right) = \exp\left(\overline{X}_i\beta\right) / \left[1 + \exp\left(\overline{X}_i\beta\right)\right]$ and $\overline{X}_i$ is a vector of firm covariates. The estimation of Eq. (12) yields parameter estimates $\hat{\beta}$, which are used to construct, for each observation, the estimated conditional density at the treatment intensity actually received, i.e., the GPS or continuous-treatment equivalent of the estimated propensity score:[31]

$$\hat{L}_i = L_i\left(\hat{\beta}\right) = \left[\Lambda\left(\overline{X}_i \hat{\beta}\right)\right]^{Di}\left[1 - \Lambda\left(\overline{X}_i \hat{\beta}\right)\right]^{1-D_i} \quad (13)$$

As in our earlier case of a binary treatment propensity score, proceeding this way eliminates selection bias in the estimated treatment effect due to differences in pre-treatment covariates at different treatment intensities (Hirano and Imbens, 2004).

In the second stage, we estimate the conditional means of an outcome variable (say, total export growth) as a function of the treatment intensity and the estimated GPS based on a flexible linear specification. Following Hirano and Imbens (2004), we use OLS to estimate the coefficients on a second-degree polynomial in the treatment intensity and the estimated GPS given by:

$$E\left(\Delta y_i / D_i, \hat{L}_i\right) = \alpha_0 + \alpha_1 D_i + \alpha_2 D_i^2 + \alpha_3 \hat{L}_i + \alpha_4 \hat{L}_i^2 + \alpha_5 D_i \hat{L}_i \quad (14)$$

The third stage consists of estimating and plotting a "dose-response" function mapping each treatment intensity into expected outcome realizations.[32] Following Newey (1994) and Bia, Flores and Mattei (2011), the dose-response function is constructed by averaging the conditional means in Eq. (14) over the distribution of the estimated GPS for each treatment intensity *d* as in:

$$\varphi(d) = \frac{1}{N}\sum_i \left[\alpha_0 + \alpha_1 d + \alpha_2 d^2 + \alpha_3 \hat{L}_i(d) + \alpha_4 \hat{L}_i(d)^2 + \alpha_5 d \hat{L}_i(d)\right] \quad (15)$$

where *N* is the total sample size. This is a highly non-linear function of *d*; indeed, one of the advantages of this methodology that it does not impose any restriction on the relationship between the continuous treatment intensity and the outcome (Fryges and Wagner, 2008). In practice, Eq. (15) is estimated numerically for each centile of the distribution of *d* in the $[0,1]$ interval. Confidence intervals for the dose-response functions are calculated by bootstrapping, re-estimating the GPS at each bootstrap to take into account the uncertainty in its estimation. Since the procedure controls for differences in pre-treatment variables, the estimated difference between the average potential outcome at two different treatment intensities—each based on Eq. (15)—is called a "pairwise treatment effect" and can be interpreted as the effect of varying treatment intensity.

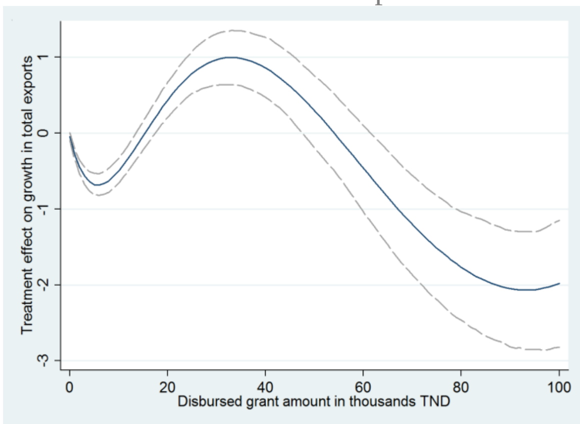
Using total export growth as the outcome, the dose-response function is shown in Figure 4 together with 90% confidence intervals. The horizontal axis measures treatment intensity centiles—*not* centiles of the distribution of firms by grant amount—so they can be read directly

[31] Eq. (12) is estimated using the generalized linear models framework of McCullagh and Nelder (1989).

[32] Hirano and Imbens (2004) argue that the regression function in Eq. (14) does not have a causal interpretation. It represents the average potential outcome for the strata defined by the GPS (the conditional density of receiving a particular treatment level) but does not allow us to compare directly outcome values for different treatment levels and obtain the causal difference in outcomes from receiving a treatment level versus another (Heinrich, Maffioli, and Vazquez, 2010). Hence, there is a need for the third stage in the GPS methodology.

as grant amounts, in thousand dinars. The shape of the function near the upper bound of the treatment should be interpreted with caution, as relatively few observations are available in that region. Be that as it may, diminishing returns seem to set in at around TND 38,000 (USD 26,000), i.e. well before the grant ceiling, and treatment effects are positive and significant only between about TND 18,000 (USD 12,300) and TND 56,000 (USD 38,200).

Figure 4
Dose-Response Function: Growth in Total Exports as a Function of Grant Size

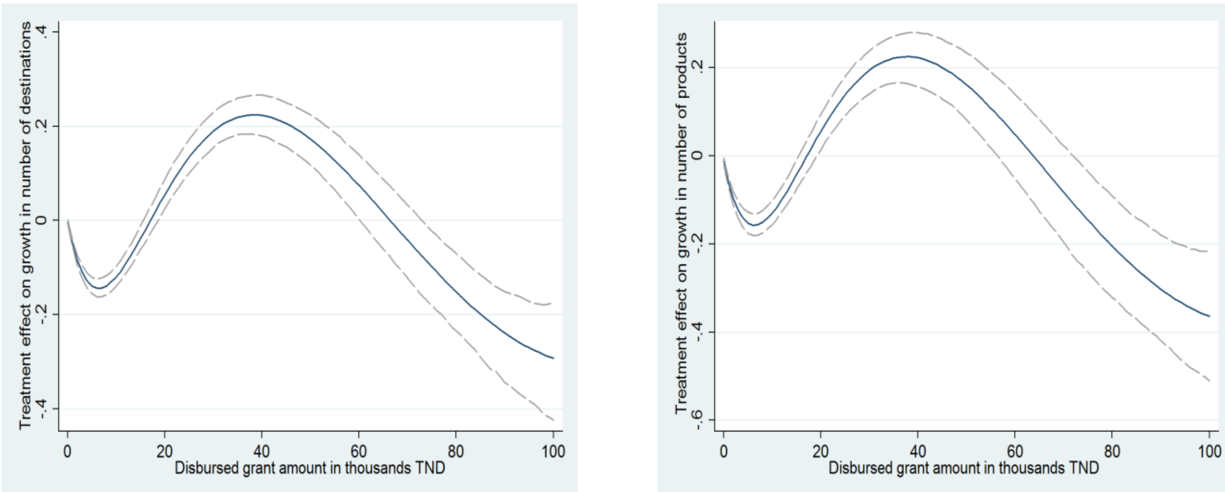


Notes: the solid line shows the estimated conditional expectation of firms' one-year growth in total exports (for FAMEX firms between the year before receiving support and the first year when support is received) given the FAMEX grant amount and the estimated GPS. The dashed lines represent the 90% confidence interval based on the bootstrap distribution.

Dose-response functions using destination growth and product growth as the outcome variables are shown in Figure 5. The shapes are similar, although estimated effects at the peak are smaller (in the vicinity of 20% for both destinations and products, against 100% for total export growth, which is consistent with the binary-treatment results of Table 4). In all cases, grants below TND 180,000 seem to serve little purpose.

Figure 5
Dose-Response Functions: Growth in Number of Destinations and Products as a Function of Grant Size

(a) Destinations (b) Products



Notes: the solid line shows the estimated conditional expectation of firms' one-year growth in the number of destinations in panel (a) or the number of products in panel (b) (for FAMEX firms between the year before receiving support and the first year when support is received) given the FAMEX grant amount and the estimated GPS. The dashed lines represent the 90% confidence interval based on the bootstrap distribution.

5.4 Externalities

Estimated treatment effects can be biased by the presence of general equilibrium effects - which not very likely in the case of an assistance program of limited scale like FAMEX - or by externalities "polluting" the outcomes of the control group[33] For instance, it might be that FAMEX beneficiaries' actions such as the participation in trade fairs or the hiring of export-marketing consultants were visible and easily imitable by other firms in their sector or location. It might even be that FAMEX beneficiaries shared information voluntarily, as exporters from the same country do not necessarily see themselves as competitors.[34] The benefits of FAMEX could be underestimated if the control group's export outcomes improved as a result of spillovers from FAMEX beneficiaries. Importantly, the presence of externalities indicates the non-appropriability of knowledge about export markets and thus would be needed to justify the intervention by the Tunisian government and the World Bank.

[33] Formally, treatment effects are measured under the assumption of "stable unit treatment value assumption" (Rubin 1961), which means that a treated individual's outcome is independent of the treatment's mode of administration and of the status of other individuals (treated or not). See Galiani and Porto (2010) for a discussion.

[34] For instance, Cadot, Iacovone, Pierola, and Rauch (2011) show that, for African exporters, one exporter's expected survival rises with the number of firms from the same country exporting the same product to the same destination.

Interestingly one interpretation for our findings in Section 5.2 of vanishing effects of FAMEX assistance after two years could be that control firms catch-up, indicating the presence of externalities. A major difficulty in investigating this issue explicitly is that the measurement of spillovers is elusive, especially when one does not know what their transmission channel is. In their classic study of a child de-worming program in Kenya, Miguel and Kremer (2004) estimated program externalities across schools by including in the outcome regressions the number of pupils in treated schools within a certain distance. In their case, this number was exogenous because of randomized school assignment.

We construct a proxy for exposure to treated firms following the same logic as Miguel and Kremer (2004), that is, we assume that externalities are more likely to exist between firms in the same sector and region. Although the case for contagion is less clear-cut in our case, we assume that firm managers are more likely to know each other if they produce similar goods in the same region. Thus, we construct a count variable equal to the number of FAMEX beneficiaries in each sector-region-year cell. This exposure to FAMEX beneficiaries is a time-variant characteristic that is available for each firm in the control group. Our reduced form approach to catch the effect of spillovers consists of estimating regressions that explain export-related outcomes based on the exposure to FAMEX beneficiaries *for control-group firms only*. Since the year of enrolment *t(i)* is not defined for control firms, we consider regressions of first differences of their outcomes in a panel framework given by:

$$\Delta y_{jsrt} = \alpha + \sum_k \beta_k n_{jsr,t-k} + \delta_j + \delta_{st} + \delta_{rt} + \nu_{jsrt} \tag{16}$$

where s and r designate, respectively, sectors and regions, $n_{jsr,t-k}$ is the number of FAMEX beneficiaries in control firm j 's sector-region in year $t-k$. Eq. (16) includes sector-year interaction fixed effects δ_{st} and region-year interaction fixed effects δ_{rt} that control for temporary shocks in a sector or region which could affect both outcomes and the number of firms receiving FAMEX support in that sector or region. The exposure variable enters with a variety of lags to deal to mitigate any potential endogeneity and more importantly to allow for the slow diffusion of externalities. Eq. (16) is estimated including firm fixed effects δ_j and the results are shown in Table 8.[35] The estimates fail to suggest any positive externalities; on the contrary, the results suggest negative effects of a larger presence of FAMEX beneficiaries on all export outcomes of control firms after one year, although only one of the effects is significant, namely the four-year effect in column (12). The evidence of negative spillovers is not strong; nevertheless, it is suggestive of the possibility that FAMEX-assisted firms use their cash to poach talented managers and workers from control firms and serves as a reminder that externalities can go either way.

[35] Results from OLS estimation without the firm fixed effects are qualitatively similar.

One could argue that spillovers to control firms are more likely to emerge from FAMEX beneficiaries whose objective was to expand into new destinations or to export new products than from FAMEX beneficiaries that are just becoming more substantive exporters. Hence, the spillovers from all FAMEX beneficiaries in Table 8 could have been under-estimated. To address this possibility we estimate Eq. (16) including a variant of the count variable equal to the number of FAMEX beneficiaries in each sector-region-year cell whose objective was to reach more destinations or export more products. The unreported results are similar to those in Table 8 and provide no evidence of externalities.

Table 8
Effect of Exposure to FAMEX Firms on Control Firms' Export Outcomes

Estimator	Firm Fixed Effects (Within)				Firm Fixed Effects (Within)				Firm Fixed Effects (Within)			
Outcome	First diff. of total exports for sample of control firms				First diff. of nb. destinations for sample of control firms				for sample of control firms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Exposure to FAMEX benef. t-1	-0.052	-0.050	-0.016	-0.122	-0.003	-0.004	0.004	0.000	-0.006	-0.006	0.000	-0.022
	(1.79)	(1.64)	(0.39)	(1.39)	(1.04)	(1.27)	(0.87)	(0.03)	(1.49)	(1.56)	(0.03)	(1.95)
Exposure to FAMEX benef. t-2		0.004	0.037	-0.019		-0.002	0.005	-0.005		-0.001	0.005	-0.020
		(0.14)	(0.85)	(0.18)		(0.75)	(1.25)	(0.47)		(0.33)	(0.83)	(1.44)
Exposure to FAMEX benef. t-3			0.012	-0.028			0.005	-0.004			0.006	-0.015
			(0.31)	(0.28)			(1.39)	(0.43)			(1.12)	(1.14)
Exposure to FAMEX benef. t-4				-0.060				-0.008				-0.022*
				(0.76)				(1.11)				(2.05)
Number of firms	2,620	2,620	2,618	2,618	2,620	2,620	2,618	2,618	2,620	2,620	2,618	2,618
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sector-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	*0.02*	*0.02*	*0.02*	*0.02*	*0.01*	*0.01*	*0.01*	*0.01*	*0.02*	*0.02*	*0.02*	*0.02*
Observations	12,785	12,785	10,316	7,802	12,785	12,785	10,316	7,802	12,785	12,785	10,316	7,802

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. The sample includes only control firms in or out of the common support.

5.5 Did Assistance Encourage Risk-Taking?

The experimental-economics literature has long noticed that individuals tend to be more willing to take risks out of windfall gains than out of regular earnings, a phenomenon called the "house money effect" (a term borrowed from gambling). For instance, Thaler and Johnson (1990) asked two groups of individuals to choose between two lotteries with the same expected value, one riskier than the other. The treatment group was given a prior endowment (the windfall) while the control group was given no endowment. They report that 77% of the treatment group's individuals chose the risky lottery against only 44% for the control group.[36] We noted in Table 3 that FAMEX firms performed worse in terms of export growth than control firms during the onset of the global financial crisis (2007-2009). Although FAMEX was a matching-grant program rather than a pure subsidy—precisely in order to limit moral hazard—could it be nevertheless that export promotion encouraged beneficiary firms to take more risk?

Before exploring this question, we provide further evidence of the diversification associated with FAMEX assistance. Tables 4 and 5 show that growth in the number of destinations and in the number of products exported is significantly and persistently higher for FAMEX firms relative to control firms. To combine the two dimensions, we construct for each Tunisian firm two standard measures of concentration - a Herfindahl index and a Theil index - of their export shares across product-destination cells – below designated as product-destination *markets* - in each year. We estimate WLS regressions as in Eq. (8) using each of the concentration measures as dependent variable and present the results in Table 9. The estimates indicate a clear increase in product-destination diversification over time for FAMEX firms relative to control firms.

To explore the possibility of increased risk-taking following FAMEX, we construct a measure of the price risk characterizing the export portfolio of Tunisian firms. The high transaction costs involved in diversifying export portfolios make them different from financial asset portfolios, so instead of using an off-the-shelf model (CAPM or other), we construct an ad-hoc measure of price risk that is independent from measurement units. We do this in two steps. In step 1, we calculate a measure of price risk in a product-destination market, treating the average unit value in that market as a random variable—i.e., assuming that our exporters are small in their export markets, which is a reasonable assumption for Tunisian firms. Let p_{kt}^{ij} be the price (CIF unit value in U.S. dollars), at time *t*, of product *k* in destination *j* when imported from origin *i*. That is, p_{kt}^{ij} is for example the price of men's cotton t-shirts imported from Vietnam into Germany in 2005. Let $\tilde{p}_{kt}^{j} = \sum_i \omega_{kt}^{ij} p_{kt}^{ij}$ be an import-weighted average of those unit values in market *j* at time *t* (ω_{kt}^{ij} is the share of origin *i* in imports of product *k* into *j* at time *t*). That is, $\tilde{p}_{kt}^{j}$ is the average price of men's cotton t-shirts imported into Germany in 2005. We define the price risk for

[36] See also Clark (2002).

product k in destination j as the variation in $\tilde{p}_{kt}^{j}$ across sending countries and over time during a reference ten-year period (2000-2009).[37] That is, we treat the German market for men's cotton t-shirts as a risky asset in which a Tunisian firm may choose to invest some of its export resources. Exchange-rate fluctuations, which are part of the price risk faced by exporters, are subsumed in the price variation since unit values are expressed in USD terms, leaving only the USD/TD exchange rate as a systemic risk affecting all portfolios symmetrically. To capture the variation in $\tilde{p}_{kt}^{j}$ we use the corresponding coefficient of variation ν_{k}^{j} in order to make our product-destination market measure of price risk independent of units of measurement.

In step 2, we aggregate the ν_{k}^{j} over all product-destination markets for a given firm, using export share weights to obtain a firm-level measure of price risk as follows. Consider a portfolio of n product-destination markets, each with average unit value $\tilde{p}_{kt}^{j}$ and fixed (non-stochastic) export weight w_{kt}^{j}. The average price risk of a firm's export portfolio is given by (ignoring the firm subscript for simplicity):

$$P_t = \sum_j \sum_k w_{kt}^{j} \tilde{p}_{kt}^{j} .$$

To simplify the notation, let us index product-destination markets by $n = 1,\ldots,N$, so w_{nt} is the weight of market n in the firm's portfolio at time t, $\tilde{p}_{nt}$ is the average unit value for that market at t, and ν_n is its coefficient of variation over the reference ten-year period. The variance of our price index can be expressed as:

$$\operatorname{Var}(P_t) = \sum_n (w_{nt})^2 \operatorname{Var}(\tilde{p}_{nt}) + 2\sum_n \sum_{m<n} w_{nt} w_{mt} \operatorname{Cov}(\tilde{p}_{nt}, \tilde{p}_{mt}) \qquad (17)$$

Variances and covariances are not independent of units of measurement, thus we need to express Eq. (17) in terms of coefficients of variation ν_n. Let σ_n be the ten-year standard error of $\tilde{p}_{nt}$, and $\bar{p}_n$ its ten-year average. Using ρ_{nmt} for the correlation coefficient between $\tilde{p}_{nt}$ and $\tilde{p}_{mt}$,

$$\begin{aligned}\operatorname{Var}(P_t) &= \sum_n (w_{nt})^2 (\bar{p}_{nt})^2 (\nu_{nt})^2 + 2\sum_n \sum_{m<n} w_{nt} w_{mt} \rho_{nmt} \sigma_{nt} \sigma_{mt} \\ &= \sum_n (w_{nt})^2 (\bar{p}_{nt})^2 (\nu_{nt})^2 + 2\sum_n \sum_{m<n} w_{nt} w_{mt} \rho_{nmt} \bar{p}_{nt} \bar{p}_{mt} \nu_{nt} \nu_{mt}\end{aligned} \qquad (18)$$

So the coefficient of variation of the price index is given by:

[37] These indexes will be recalculated and updated to cover the entire sample period 2000-2010 using unit value data from CEPII.

$$\nu_t = \sqrt{\sum_n \left(\frac{w_{nt}\bar{p}_{nt}}{\bar{p}_t}\right)^2 (\nu_{nt})^2 + 2\sum_n \sum_{m<n} \frac{w_{nt}\bar{p}_{nt} w_{mt}\bar{p}_{mt}}{(\bar{p}_t)^2} \nu_{nt}\nu_{mt}\rho_{nmt}}$$

$$= \sqrt{\sum_n W_{nt}^{\;2} (\nu_{nt})^2 + 2\sum_n \sum_{m<n} W_{nt} W_{mt} \nu_{nt}\nu_{mt}\rho_{nmt}} \qquad (19)$$

where the W_{nt}'s are value (dollar) weights. Recall that the coefficient of variation in Eq. (23) is firm-specific but its only source of time-wise variation is the weights W_{nt} chosen by the firm. Therefore, Eq. (19) picks up pure composition effects in the firms' resource allocation decisions. This index, which is independent of the units in which trade flows are measured, is our proxy for the price risk faced by Tunisian firms in their export portfolios and its dynamics show how it evolved over time as a result of their decisions. We estimate WLS regressions as in Eq. (8) to assess the effects of FAMEX assistance on that proxy for price risk. The results reported in Table 9 suggest that FAMEX beneficiaries did not take on more risk than control firms. [38]

Two possible reasons for why price risk did not decrease for FAMEX firms even though they diversified their portfolios could be that (i) they diversified into riskier product-destination markets, or (ii) they diversified into markets whose price movements were highly correlated with their existing ones. To explore these possibilities we estimate WLS regressions as in Eq. (8) on a decomposition of the variance and the covariance components of Eq. (18). The results in Table 9 are not clear-cut, but suggest that while the portfolio covariance component was not significantly higher for FAMEX beneficiaries, its variance component (price risk) seems to have increased significantly two years after FAMEX enrolment.

[38] Note that the results in Table 9 are based on a smaller sample of 271 FAMEX firms and 1467 control firms. The reason for the sample size reduction is that the portfolio price risk index measure cannot be calculated for product-destination markets for which less than 10 years of unit value data are available.

Table 9
Effects of FAMEX on Concentration Indexes and on Portfolio Price Risk

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg. (1)	WLS reg. (2)	WLS reg. (3)	WLS reg. (4)	WLS reg. (5)	WLS reg. (6)
Outcome						
Herfindahl index	-0.131	-0.108	-0.142	-0.153	-0.144	-0.267
	(-4.34)***	(-2.73)***	(-3.42)***	(-3.34)***	(-2.57)**	(-3.91)***
R-squared	*0.076*	*0.105*	*0.090*	*0.098*	*0.102*	*0.126*
Theil index	-0.021	-0.020	-0.025	-0.025	-0.026	-0.046
	(-4.83)***	(-3.42)***	(-4.13)***	(-3.72)***	(-3.17)***	(-4.38)***
R-squared	*0.102*	*0.134*	*0.117*	*0.121*	*0.143*	*0.150*
Covariance index	-0.031	-0.018	0.019	-0.043	0.078	
	(-0.49)	(-0.39)	(0.44)	(-0.70)	(0.90)	
R-squared	*0.032*	*0.025*	*0.026*	*0.017*	*0.033*	
Variance index	-0.272	-0.038	0.584	-0.308	0.454	
	(-1.21)	(-0.18)	(2.81)***	(-1.24)	(1.30)	
R-squared	*0.035*	*0.026*	*0.033*	*0.024*	*0.024*	

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. The sample includes treated firms and control firms in the common support. See the text for the definition of the outcome variables. The WLS regressions include treatment year fixed effects and the following firm covariates: firm age and age squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial total exports, number of exported products and number of destinations served.

6. Robustness and Extensions

6.1 Year-by-Year Matching

The PSM-DID results shown in Table 4 are generated by a procedure allowing a given FAMEX beneficiary whose enrolment in the program takes place in year *t(i)* to be matched to a combination of control firms drawn in year *t(i)* but also in years prior to *t(i)* or after *t(i)*. This procedure poses serious problems for an unbiased measurement of the impact of FAMEX, as matching is based solely on time-invariant firm characteristics. Hence, a firm j that is a good match for firm i (in the sense of having a similar propensity score) may be very different from firm i in terms of outcomes when firm i is observed at time *t(i)* while firm j is observed at a different time period. For instance, if *t(i)* is a 'bad' year and the control firm is observed in a 'good' year, then the treatment effect will be underestimated. In our WLS regressions, macroeconomic shocks are controlled for by the inclusion of time fixed effects, but such controls are not used in the PSM-DID estimator. For better comparability with WLS estimates, the PSM-DID estimates need to be adjusted.[39]

We consider an alternative type of PSM which pairs each treated firm with a set of control firms in the same year that the firm enrolls into FAMEX *t(i)* following the procedure employed by Todo (2011).[40] Using the same propensity scores as used in Section 5.1 (based on the probit regression in Appendix Table A.1), this matching procedure generates for each treated firm in year *t(i)* a 'fictitious' control firm which is a weighted average of control firms with close propensity scores all observed in year *t(i)* and computes an outcome for that 'fictitious' control firm in year *t(i)*.[41] This procedure results in a new cross-sectional dataset per year where each treated firm is matched with a unique fictitious control firm for which a generated outcome is measured. By construction, the size of the dataset for a given year is twice the number of firms for which that year was the first year of FAMEX support. We pool across all treatment years (2005-2009) and estimate different variants of OLS DID regressions of export outcomes on a FAMEX treatment indicator. Panel A presents results that are comparable to those in Table 4 while Panel B presents results that are comparable to those in Table 5.[42]

The results in column (1) reveal a strong positive and significant effect of FAMEX on export growth both at the intensive as well as the extensive margins in the first year when FAMEX support is received. These estimates are in fact comparable to the matching DID estimates in Table 4 suggesting that the PSM within each year does not modify the pattern of a strong immediate impact as the matching grant is received. Columns (2)-(5) show significant treatment effects in most years after the year when FAMEX support is first received for the number of destinations and products but only short-lived effects for total exports.

[39] Arnold and Javorcik (2009) argue that in constructing matched pairs of treated firms-control firms based on the propensity score it is important to ensure that the matched observations are assigned from the same year and sector to eliminate the possibility that differences in unobservable dimensions of firm performance across different year-sector combinations exert influence on the estimated PSM-DID effects.
[40] Due to the relatively small number of exporters in each sector shown in Appendix Table A.1, we are unable to consider sector in this type of PSM.
[41] The unreported balancing tests corresponding to this matching are satisfactory.
[42] For simplicity we omit the R-squares from the regressions shown in Table 10.

Table 10
Effects of FAMEX on Export Outcomes using Year-by-Year Matching

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	DID reg.	DID reg.	DID reg.	DID reg.	DID reg.	DID reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.503	0.689	0.486	0.139	-0.25	-0.148
	(2.95)***	(3.18)***	(2.10)**	(0.51)	(-0.71)	(-0.37)
Nb. destinations	0.135	0.161	0.147	0.100	0.076	0.100
	(5.35)***	(5.64)***	(4.51)***	(2.69)***	(1.54)	(1.81)*
Nb. products	0.139	0.155	0.137	0.058	0.094	0.130
	(4.32)***	(3.97)***	(3.27)***	(1.17)	(1.56)	(1.99)**
Observations	802	802	798	716	560	516
Treated	401	401	399	359	280	258

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The DID regressions include treatment year effects.

6.2 FAMEX Drop-Outs

The estimates for the FAMEX effect presented in Section 5 and in Section 6.1 include as part of the control group 126 firms that applied and were accepted into the FAMEX program but subsequently dropped out of the program. If those firms are more similar to FAMEX recipients than other control firms, their inclusion in the control group should improve the quality of the matching and subsequently the accuracy of the estimates of the treatment effect. One could in fact think of an ideal specification where dropouts would be the only firms in the control group. Such specification would however be difficult to estimate given the small number of dropout firms relative to the number of FAMEX firms. Hence, instead of that specification we estimate Eq. (8) using alternative samples where we either (A) completely eliminate the drop-outs from the sample or (B) include the drop-outs in the treatment group.[43] The results are presented in Table 11.

The magnitude of the treatment effects in Panel A where dropouts are excluded from the sample is slightly larger than that of the corresponding treatment effects in Table 5. In Panel B where dropouts are included in the treated group the estimates indicate generally a smaller effect of FAMEX assistance.[44] In combination, the findings in Panels A and B suggest that dropout firms performed better than other firms in the control group (hence once they are excluded from the

[43] In Panel A our sample includes 401 treated firms and 2220 control firms while in Panel B our sample includes 526 treated firms and 2220 control firms.
[44] An exception to this pattern is the higher effect on total exports in column (1) of Panel B of Table 11 relative to column (1) of Table 5.

control group a stronger beneficial effect of FAMEX is identified) but performed worse than FAMEX firms that achieved their export business plan.

Table 11
Effects of FAMEX on Export Outcomes – Alternatives for Dropouts

Panel A. Excluding Dropouts from the Sample

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.	WLS reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.548	0.725	0.517	0.098	-0.138	0.033
	(3.26)***	(3.59)***	(2.32)**	(0.36)	(-0.40)	(0.08)
Nb. destinations	0.154	0.186	0.179	0.113	0.108	0.141
	(6.18)***	(6.73)***	(5.54)***	(3.15)***	(2.30)**	(2.60)***
Nb. products	0.150	0.175	0.167	0.079	0.120	0.176
	(4.74)***	(4.68)***	(4.14)***	(1.69)*	(2.02)**	(2.67)***
Observations	11,645	11,645	9,423	7,160	4,846	2,537

Panel B. Including Dropouts in the Treated Group

Difference	TY-(TY-1)	(TY+1)-(TY-1)	(TY+2)-(TY-1)	(TY+3)-(TY-1)	(TY+4)-(TY-1)	(TY+5)-(TY-1)
Estimator	WLS reg.	WLS reg.	DID reg.	DID reg.	DID reg.	DID reg.
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome						
Total exports	0.522	0.590	0.273	0.091	-0.238	-0.007
	(3.32)***	(3.15)***	(1.32)	(0.37)	(-0.75)	(-0.20)
Nb. destinations	0.139	0.143	0.140	0.091	0.090	0.134
	(6.15)***	(5.63)***	(4.81)***	(2.80)***	(2.07)**	(2.70)***
Nb. products	0.116	0.140	0.124	0.069	0.088	0.173
	(4.17)***	(4.31)***	(3.46)***	(1.67)*	(1.63)	(2.88)***
Observations	11,950	11,950	9,719	7,421	5,038	2,656

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The WLS regressions include treatment year fixed effects and the following firm covariates: firm age and age squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial total exports, number of exported products and number of destinations served.

6.3 FAMEX Grades and Grant Consumption Rates

The FAMEX program administration team assigned ex-post to each beneficiary firm a grade ranking the performance of its co-financed export business plan. The grades were as follows: A for successful performer, B for medium performer and C for poor performer. To examine the extent to which FAMEX grades corresponded with real export performance we re-estimate Eq.

(8) (for the year of FAMEX enrolment) interacting the treatment indicator with the grade assigned to the export business plan. The data on FAMEX grades is available for 287 FAMEX beneficiaries, of which 169 received an A grade, 64 received a B grade, and 54 received a C grade. The results are presented in Table 12 and show a strong positive and significant effect of FAMEX on export growth at the intensive and extensive margin for firms whose export business plan was rated A. Weak positive effects are found for growth in destinations for firms whose business plan was rated B and for growth in products for firms whose business plan was rated C. Given that the grades were generally awarded in the first year after firms joined FAMEX as projects were assessed, it is not surprising to see a clean and strong correspondence between grade A and good export outcomes in the year of FAMEX enrolment since FAMEX administrators could likely have observed the firms' export outcomes.

Table 12
FAMEX Grades and Export Outcomes

Difference	TY-(TY-1)	TY-(TY-1)	TY-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.
Outcome	Total exports (1)	Nb. destinations (2)	Nb. products (3)
FAMEX Grade			
High Performer:A	0.993	0.239	0.209
	(3.99)***	(6.05)***	(4.15)***
Medium Performer: B	0.269	0.112*	0.031
	(0.59)	(1.78)	(0.44)
Low Performer: C	0.24	0.048	0.154
	(0.64)	(0.99)	(1.81)*
R-squared	*0.21*	*0.19*	*0.17*
Observations	12,149	12,149	12,149

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The WLS regressions include treatment year fixed effects as well as the following firm covariates: firm age and age squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial total exports, number of exported products and number of destinations served.

FAMEX beneficiaries were awarded a certain grant amount to co-finance their export business plan, but the grant amount was disbursed to firms only upon submission of receipts for reimbursement of the expenses incurred in to execute the business plan. In Section 5.3 we examined the effects of different grants disbursed on export outcomes. Here we consider whether the grant consumption rate - defined as the share of the awarded grant that was effectively disbursed to a firm - affects export outcomes. Since grant disbursements and consumption rates are calculated only after Tunisian firms' projects are closed, a grant consumption rate below 100% indicates that the firm decided not to pursue some of its planned activities because it judged them to be non-necessary, because it already achieved its objectives, or because it does not have the funds to cover the required 50% co-financing. We re-estimate Eq. (8) (for the year of FAMEX enrolment) interacting the treatment indicator with an indicator variable identifying the grant consumption bin to which the beneficiary firm belongs and show the results in Table 13. The bins are as follows: low consumption includes 105 firms whose grant utilization is lower than one-third of the awarded grant amount, medium consumption includes 126 firms whose grant utilization ranges between one third and two-thirds of the awarded grant amount, and high consumption includes 113 firms whose grant consumption is higher than two-thirds of the awarded grant amount.

The estimates show positive and significant treatment effects on exports at the intensive and extensive margins for FAMEX beneficiaries who consumed less than two-thirds of their grant. Interestingly the treatment effects are larger the smaller is the consumption rate. Firms that consumed more than two-thirds of the grant exhibit a significantly higher number of destinations and number of products exported than control firms but their total export growth is no higher than that of control firms. In order to interpret this result, recall that whereas firms submit general export business plans, the lists of specific activities eligible for matching grants are drawn up by the FAMEX program administration team. It seems plausible that some items on those lists are more relevant than others, which the FAMEX team is likely to add in order to make the plans look complete. Competent managers are likely to be more selective in their choice of activities and thus to stop below 100% grant consumption rates, whereas very high consumption rates may reflect inexperienced management.

Table 13
FAMEX Grant Consumption and Export Outcomes

Difference	TY-(TY-1)	TY-(TY-1)	TY-(TY-1)
Estimator	WLS reg.	WLS reg.	WLS reg.
Outcome	Total exports (1)	Nb. destinations (2)	Nb. products (3)
Grant Consumption			
Low Consumption (less than 1/3 of grant)	1.147***	0.185***	0.191***
	[3.39]	[3.84]	[3.09]
Medium consumption (between 1/3 and 2/3 of grant)	0.794***	0.177***	0.183***
	[3.34]	[4.38]	[4.21]
High consumption (more than 2/3 of grant)	0.365	0.166***	0.144**
	[1.11]	[3.37]	[2.13]
R-squared	*0.21*	*0.18*	*0.16*
Observations	12,206	12,206	12,206

Notes: T-statistics based on robust standard errors in parentheses;* significant at 10%; ** significant at 5%; *** significant at 1%. TY is the first year when FAMEX support is received. The WLS regressions include treatment year fixed effects and the following firm covariates: firm age and age squared, location and sector fixed effects, a categorical variable for firm size in terms of employment, a dummy variable identifying whether the firm exports 100% of its output, as well as initial total exports, number of exported products and number of destinations served.

7. Concluding Remarks

Our study estimates a stronger impact of the export promotion program – FAMEX - at the extensive margin than at the intensive margin, and a stronger impact in terms of geographical diversification than in terms of product diversification. This pattern mirrors the findings in a previous study on export promotion by Volpe and Carballo (2008). Moreover, we find that the effect on export growth rates is only transient. However, our results on the long-term impact of export promotion must be interpreted cautiously given that the later years of our sample period are special in that they were characterized by the collapse of world trade, which may not have affected all firms equally. In particular treated firms may have ventured into riskier markets.

In terms of policy implications, the quasi-experimental approach suffers from a fundamental drawback, namely, that treatment effects, which are generally construed as favorable to policy intervention when they are significant, only indicate appropriable benefits. They give no indication on the presence of a market failure. Indeed, if anything, it is the absence of treatment effect that is consistent with the non-appropriability of benefits. We attempt to go around this problem by testing directly for the presence of externalities. We find the opposite of the positive externalities that could justify government intervention: in fact, the spillovers, if anything, are negative, possibly reflecting poaching of high-performance workers and managers by assisted firms with the help of public funds. Thus, even though we do find significant treatment effects, the policy implications of our findings are not unambiguously favorable to public funding of export promotion.

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Appendix

Appendix A: Propensity Score Estimation

Table A.1 Probit Regression for the Propensity to receive FAMEX treatment

	FAMEX treatment status
Age	0.355
	(0.354)
Age squared	-0.098
	(0.065)
Total exports in 2004	-0.038
	(0.009)***
Number of exported products in 2004	0.158
	(0.046)***
Number of export destinations in 2004	0.497
	(0.058)***
100% exporter	-0.341
	(0.070)***
10-19 employees	-0.491
	(0.116)***
20-49 employees	-0.359
	(0.099)***
50-99 employees	-0.393
	(0.106)***
100-199 employees	-0.385
	(0.113)***
More than 200 employees	-0.411
	(0.122)***
Textiles and apparels	-0.067
	(0.088)
Paper, wood, and furniture	0.019
	(0.102)
Chemicals	-0.041
	(0.100)
Metals	-0.021
	(0.116)
Machine and equipment	0.017
	(0.099)
Electric	-0.111
	(0.122)
Grand Tunis	-0.352
	(0.072)***
Central Sea	-0.950
	(0.157)***
Rest of Tunisia	-0.447
	(0.077)***
Year fixed effects	Yes
Observations	12,263

Notes: Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%. Unless noted, firm characteristics refer to 2007. The omitted sector is agro-industry, the omitted location is Tunis, and the omitted category in terms of employment is less than 10 workers.

Annex B: Balancing Tests for the Propensity Score Matching

To assess the quality of the propensity score matching in balancing adequately the covariates between treatment and control groups, we conduct four types of tests. The first test is the balancing or stratification test proposed by Dehejia and Wahba (2002) which divides observations into strata based on the estimated propensity score and uses t-tests within each strata to test if the distribution of covariates is similar between the treatment and control group. such that there are no statistical differences between the mean of the propensity score in the treatment and control group. Implementing the test in stata as in Becker and Ichino (2002) over 6 strata of the propensity score shows that the balancing property is satisfied for our data.

The second set of tests shown in the first columns of Appendix Table A.2 consists in two-sample t-tests for the equality of sample means for all the covariates between treated and matched control groups. The t-tests indicate no significant differences in the means suggesting that the covariates are balanced in the two groups and thus the quality of our matching is high.

The third set of tests shown in the last columns of Appendix Table A.2 are the standardized biases for the covariates defined as the corresponding difference in sample means between treated and matched control groups normalized by the square root of the average of sample variances in both groups. The results show that the standardized bias for our covariates is in most cases lower than 5%. Caliendo and Kopeining (2008) suggest that a standardized bias of that magnitude after matching indicates high quality of the matching.

The fourth test is based on the comparison of the pseudo-R-squared of the propensity score estimated on the full sample versus on the matched sample, which explains how well the covariates explain the propensity to participate in the program. With a high quality matching, the pseudo-R-squared should be very low after matching because there should be no differences in the distribution of the covariates that can explain the propensity to participate in the program. Indeed, our pseudo-R-squared is 0.086 before matching and 0.003 after matching. Moreover, the associated likelihood-ratio test of the joint insignificance of covariates in the propensity score estimation on the full sample versus on the matched sample should indicate that the covariates are jointly insignificant in explaining participation after matching. Indeed our likelihood-ratio chi-squared test is 316.85 with a p-value of 0 before matching and 3.64 with a p-value of 1 after matching.

Table B.1 Balancing Tests

Covariates	Mean in Matched Sample: Treatment	Control	T-test: T-statistic	P-value	Percentage Bias	Percentage Bias Reduction
Age	2.720	2.709	0.26	0.796	1.7	86.3
Age squared	7.716	7.663	0.24	0.809	1.7	83.9
Total exports in 2004	10.343	10.086	0.63	0.531	4.3	77.3
Number of exported products in 2004	1.322	1.276	0.69	0.492	5.1	84.6
Number of export destinations in 2004	1.151	1.087	1.08	0.28	8.6	83.5
100% exporter	1.340	1.362	-0.65	0.516	-4.5	84.5
10-19 employees	0.095	0.101	-0.29	0.771	-1.9	82.6
20-49 employees	0.288	0.285	0.08	0.936	0.6	80.5
50-99 employees	0.193	0.204	-0.39	0.694	-2.7	51.1
100-199 employees	0.163	0.158	0.19	0.851	1.3	-206.2
More than 200 employees	0.158	0.150	0.28	0.777	2.1	84.1
Textiles and apparels	0.315	0.330	-0.46	0.645	-3.2	85.5
Paper, wood, and furniture	0.128	0.119	0.36	0.719	2.7	73.7
Chemicals	0.125	0.132	-0.29	0.771	-2.1	27.2
Metals	0.083	0.079	0.19	0.851	1.4	81.4
Machine and equipment	0.138	0.132	0.21	0.835	1.5	83.4
Electric	0.065	0.064	0.06	0.953	0.4	89.3
Grand Tunis	0.475	0.476	-0.02	0.987	-0.1	97.6
Central Sea	0.020	0.031	-1	0.316	-5.3	80.2
Rest of Tunisia	0.283	0.287	-0.14	0.887	-1	94.6